Electric Switch and Controlling Gear: A Handbook on the Design, Manufacture, and Use of Switchgear and Switchboards in Central Stations, Factories, and Mines. By Dr. C. C. Garrard. Second edition, revised and enlarged. Pp. xxii + 654. (London: Benn Brothers, Ltd., 1920.) Price 25*s*. net.

No considerable alterations have been made in this work since the first edition was reviewed in NATURE of March 1, 1917. Slight modifications have been effected, and recent data in connection with high-tension gear, lightning arresters, etc., added. Two new sections, one dealing with the standardisation of switchgear and the other with automatic contactor switches, have also been inserted.

Milk Testing: A Simple Practical Handbook for Dairy Farmers, Estate Agents, Creamery Managers, Milk Distributors, and Consumers. By C. W. Walker-Tisdale. Second revised edition. Pp. 90. (London: J. North, *Dairy World* Office, 1920.) Price 3*s*. 6*d*. net.

THE recognition of the value and importance of "milk recording" is making it increasingly necessary that simple but trustworthy methods of testing milk should be published for the use of practical farmers. This need is well met by the present edition of Mr. Walker-Tisdale's little book, which has been enlarged and revised since the second edition was noticed in NATURE of August 10, 1911.

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Energy of Cyclones.

THERE can be no doubt, I suppose, that solar and terrestrial radiation are ultimately responsible for the kinetic energy of the winds. The doubts expressed by Mr. R. M. Deeley in NATURE of November 11 and by Mr. W. H. Dines in the issue of November 18 can refer only to the details of the phenomena consequent on the process of transformation of the energy. The first stage is obviously the storage of energy in the potential form of air charged with heat and moisture at the surface or lower levels and cooled by radiation at high levels, especially in the polar regions, as on the plateau of Greenland or on that of the Antarctic continent, or on the sunless slopes of the Himalaya. Equally without doubt the next step is convection, the greater part of which is indicated here and there by falling rain or snow. Measurements of rainfall assure us that there is no lack of energy available for violent winds if the heat-engine is at all efficient.

The general effect of the process of convection is the development of a vast circulation in the upper regions of the atmosphere from west to east round the poles, which has its counterpart in the normal distribution of pressure at corresponding levels. That is probably most pronounced at a level of 8 km., because at that level density is equal all over the globe at all seasons of the year. Above that level, up to the level of equal pressure at 20 km. of which Mr. Dines writes, there is, on the average, a gradient of density from the equator to the pole, and below the level of 8 km. a gradient of density in the opposite direction. The layer of maximum average velocity is above the layer of maximum pressure-gradient on account of the diminution of density with height.

Below the level of 8 km. the distribution of pressure is affected by the gradient of density in a very irregular manner, because the distribution of land and water is irregular. The net result at the surface is the complicated distribution of average pressure which we find in the maps of normals for sea-level.

The maintenance of the average general circulation from west to east in the higher levels is due to the gradual convergence towards the polar areas from which the cooled air flows. That must obviously be balanced by a corresponding flow towards the equator, and as poleward flowing entails a westerly circulation, so flowing towards the equator entails an easterly one. We must, therefore, find room in the system for a body of air flowing from the east comparable at least with the circulation from the west. We find such a body of air in the great easterly circulation of the intertropical regions, which is naturally stowed away over the equator as far as possible from the centres of the two polar demi-hemispheres of influence of pressure-gradient.

These great circulations, easterly and westerly, form a normal "groundwork" of all atmospheric motion; and when Mr. Deeley and Mr. Dines write of the energy of cyclones, they are not concerned, I think, with the energy of the general circulation of the upper levels which I have described, but with the minor circulations which represent the perturbations of the major circulation.

I think myself that the convection of warm, moist air, combined with the vagaries of temperature in the lower layers, will, in the end, prove to be sufficient to explain the energy of cyclonic air-currents—whether directly or as the secondary effect of current-differences, I cannot say. Probably, in order to get a correct view of the perturbations, we ought to subtract vectorially from the observed winds the local motion of the normal circulation, or else accustom ourselves more than we do to the theoretical combination of local circulation with a general circulation.

There are four other aspects of the problem upon which we are at present almost uninformed. The first is the locality where the cyclone, which is the subject of study, was generated; just as the cyclone itself is a perturbation of the general circulation, so what we see going on over our heads is the perturbation of a cyclone which may have originated in the general circulation thousands of miles away. A cyclone is a more or less stable dynamical system which certainly travels, but changes as it travels. The second aspect is the variation of velocity of the wind with height in the general circulation and in the cyclonic area itself. The third, which is closely connected with the second, is the trajectory of convected air. This could be calculated if we knew the point from which it started and the variation with height of the current which carried it. One often reads of convected air rising *vertically*, but we know that the actual trajectories of a pilot-balloon are of very various shapes, seldom vertical, and the balloon may part company from the air which supported it at the start by a distance measured in tens of kilometres. Air in convection rises very slowly. If we set its vertical velocity at one-hundredth of that of a pilot-balloon, the convected air may be thousands of kilometres from the starting point before its upward journey is finished, and its path may be very com-

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plicated. It is possible that this conception of the slow, gradual ascent of air may have a bearing upon the cloud-formation associated with a coming cyclone, but the subject is too long for a letter.

The fourth aspect is the behaviour of the convected air with regard to its environment. The slowness of its rate of ascent is dependent largely upon the development of eddies and consequent dilution of its mass with the cooler environment. This cannot of itself arrest the upward motion, though it delays it, and, consequently, when the convected air has arrived at its ultimate level it will have carried with it some of the air which formed its environment on the way. Hence the rising air will have "evicted" a certain amount of air by its passage.

The importance of combining these aspects is at once apparent if we consider that convection in still air would simply mean a readjustment of the mass in the vertical. The potentially warm air would be at the top instead of at the bottom, and the effect of a completed process of convection would be that pressure would rise within the area of operations. But if the risen air were delivered into a rapidly moving current at the top, the air which it had "evicted" from the environment on its way would be lost to the column, and when the process was completed the air would close in from the top, the bottom, and the sides. If there were any relative motion to begin with—and there is always some—closing in from the sides must develop cyclonic circulation with a cold core. Closing in from the bottom with air colder and drier than that which began the convection would stop on account of dynamical cooling, and closing in from the top means the settling down of the air of the stratosphere and a consequent low tropopause with a column of air above it warmer than its environment.

These conditions describe what the late Lord Rayleigh postulated for superposing a vortex on a current with relative velocity of its parts. They also agree with what Mr. Dines describes as the results of his examination of actual cyclonic conditions in England. And this view of the procedure is borne out by the examination of tropical cyclones. We can form legitimate inferences from the pressure records of these visitations because the normal conditions of the localities where they occur are extremely regular. We can see by an inspection of the graph of pressure that the region covered by a cyclone has simply lost a certain part of the air which it normally possesses. In one example I estimated the loss as equivalent to 40,000 cubic km. at sea-level. Beyond all doubt or question air had gone; it was not piled up in anticyclones fore and aft, as we used to think the convected air of our cyclones must be; it was gone clean away. I suspect that it travelled away in some upper current until slowed down over the tropical anticyclone of some ocean. The story will not be complete until that surmise is verified or the correct account substituted. Hence, for the time being, I am as curious about the life-history of convective air-currents as I was twenty years ago about that of surface air-currents.

In any case, it seems to me certain that, because it carries away part of the air which it meets on its path, convection, wherever it occurs, must entail convergence, and therefore, except at the equator, it must give rise to a cyclonic circulation which may be transient or, if circumstances are favourable, permanent. The function of the stratosphere seems to be not constructive, but conservative and registrative. It protects the energy from being dissipated by "filling up," because the descent of its isothermal air is arrested by the adiabatic rise of temperature. That is, indeed, the common function of all "decks" or lids in the atmosphere, of which the stratosphere is the chief. At the same time, for an observer the stratosphere registers the locality of low pressure by the lowness of the tropopause and the relative warmth of the air column above it. It seems to be a law for the general circulation and for local circulations that as pressure diminishes in the troposphere the tropopause is lowered and the temperature of the columns above it rises.

Consequently, my view at the present time is that the energy of a cyclone is due originally to convection in a region with a suitable law of variation of velocity with height; it is guarded at the top by the isothermal condition of the stratosphere, and on the sides by the balance of pressure and rotation. It is open to slow attack at the bottom on account of the friction of its winds with the surface, and unless its energy can be maintained by additional convection it must perish. I do not think that a travelling cyclone carries its supply of rain for long distances; it probably manufactures it out of the material in the lowest levels which it has to pass over. But it uses the energy so supplied first to form a secondary, and afterwards to absorb it or to be absorbed by it.

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IT is a well-known hydrodynamical result that, in the absence of any external stabilising influence, any surface of discontinuity of velocity in a fluid must be unstable. The effect of this instability is seen in the eddies produced in a millpond, at the margin of the entering stream. A sufficiently rapid shearing, without actual discontinuity, will produce the same effect. Most atmospheric eddies are developed in this way. In the case of differences of velocity between different masses of air at the same level, gravity is not directly available to damp any eddies that may be produced, and hence it does not seem likely to be difficult to account for eddies with their axes vertical.

Thus the origin of cyclones may well be explained on the lines suggested in Mr. W. H. Dines's letter in NATURE of November 18. It is rather more difficult to see what determines the size and intensity to which they grow. Ground friction must play its part; also, where the warm stream on the south side bulges northward, it must do so to some extent over the top of the cold air already there, and this arrangement makes for stability, and when sufficiently developed must prevent the further growth of the disturbance.

The speed of translation of the cyclone on this theory should be the mean of the velocities of the two currents, which is usually about correct. The geostrophic condition must also hold approximately, otherwise the disturbance would spread out with nearly the velocity of sound and disappear. What is not easy to see, however, is why the isobars tend to become more or less circular instead of wavy.

HAROLD JEFFREYS.

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I SHOULD like to express my agreement with Mr. W. H. Dines's view (NATURE, November 18, p. 375) regarding the origin of the *initial* difference of pressure which leads to the development, under the influence of the earth's rotation, of cyclonic circulation, and to state that I have often suggested that this initial disturbance may have a mechanical origin (see Quart. Journ. Roy. Meteor. Soc., vol. xliii., 1917, p. 27). At the same time it seems that one cannot, on many grounds, ignore the effect of temperature contrasts as

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